Advice to Students:

AN ADDRESS

DELIVERED AT THE

OPENING OF THE MEDICAL LECTURES OF HARVARD UNIVERSITY,

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ADDRESS.

GENTLEMEN, — It is my pleasant duty to extend to you, in the name of the Faculty, the most hearty welcome. May your studies be pursued successfully, and lead you to a respectable and respected position in your future career as medical practitioners!

I propose to give you some advice in regard to your studies now and hereafter, pointing out especially how and what you should study, what great errors and mistakes you should avoid, and what you might do for the benefit of both science and practice, if you felt inclined to devote some little time to such a noble work, during your student's life and afterwards.

I will first call your attention to some remarks which seem to me to be of such vital importance, that I hope you will try to keep them always present to your mind. I will first state that I acknowledge that many men occupying high positions in the medical profession owe these positions more or less to

mere chances, and to exceptionally favorable circumstances; but it is not the less true, — and this is what I wish you to remember, — that those persons who really have a will do succeed in making their own path, even sometimes in spite of the greatest difficulties. Applying this statement to you, I will say that your success as students, and your future success as practitioners, depend chiefly on yourselves. We of this Faculty will guide you as well as we can in your studies; but I pray you, instead of relying on us, to place your reliance chiefly in your own well-directed, energetic, and unwavering efforts.

Were all your professors the most eminent teachers of the various branches of medicine that have ever existed in this world, their talent, even backed by the greatest good-will and by constant attempts to help you, would be of no avail to you if you were not really active, earnest, and laborious students. We would try vainly to imprint knowledge on the gray matter of your brains, if you were not *actively* working yourself for its reception. The weakest man can shake the whole world by stamping the ground with his minute foot; but he cannot, however intelligent you may suppose him to be, force knowledge to fix itself in the mind of another man, unless there is some active work in this mind for the reception of knowledge. You must, therefore, attentively *listen* to your lecturers, and not simply *hear* what they say; you must read with your full powers of thought, and

not in that passive, slovenly way which consists in uttering to ourselves sentences after sentences, hardly catching a part of their meaning, so that a whole book may be soon run over, leaving in the mind of the reader nothing but a vain and obscure remembrance of the general topics of the work, and no real, profitable, and lasting knowledge.

I must, on the other hand, guard you against a fault that some students, and especially the most active-minded, will commit when hearing a lecture. They allow themselves to go on thinking of something important or interesting mentioned or suggested by the lecturer; and, following their own train of thoughts, instead of that of the teacher, they lose a part of the lecture, and sometimes nearly the whole of it.

The amount of learning you have to acquire is immense, and there is but a very short time for this work before your graduation. You must, therefore, make a methodical disposition of your time, and decide at once what will be the work of each day.

There is a most useful method of fixing knowledge in our mind, — a method which is perhaps superior to any other, and that is to communicate to others your freshly acquired notions. This you can do by forming clubs of three or four, who will meet every day, or every other day; each one being prepared to deliver a short lecture to the others on a freshly studied subject. In Paris, the students who are preparing to

compete for the posts of house-physician or house-surgeon at the various hospitals, follow, for one, two, or three years, that excellent mode of learning before they succeed to obtain, after a public show of their knowledge, the hospital posts for which there is so great a competition. Able foreign medical men, who have heard some of these students speak as if they were learned professors, throwing light on the most difficult anatomical, physiological, medical, or surgical questions, have often expressed to me their wonder at such an exhibition of thorough knowledge. Instead of feeling any surprise in that respect, I consider that this knowledge is a most natural result of the method followed to acquire it.

To conclude with the first of the rules to which I intend to call your attention, I will repeat that your success as students depends chiefly on your own activity; and I will add, that your future success as practitioners will be in a great measure in proportion to the activity, the persistence, and the good direction of your efforts previous to your graduation.

I pass to a second rule, which is of hardly less importance. It is that you must study nature at least as much as books. This principle is so obvious as regards anatomy (normal or morbid), and also as regards medicine, surgery, and obstetrics, that I need not insist upon it. But it may not be so evident to you as regards either physiology or the knowledge of the action of remedies and poisons; and I will therefore

say a few words about it. The advantages of a practical study of physiology, and of the mode of action of remedial or toxic substances, are — 1st, A greater facility of learning; 2d, The acquirement of much more positive and much clearer notions; 3d, A much more lasting remembrance, not only of all that has been seen, smelt, heard, or touched, but of the facts, theories, or laws connected with the knowledge acquired practically, *i.e.*, with the help of the senses.

I confess that I cannot understand how any one can have a clear idea of the rhythmical movements of the heart, or the peristaltic movements of the bowels, — of the rapidity and extent of the effects of the irritation or the paralysis of certain nerves upon either the quantity or the color of the blood in blood-vessels, or upon animal heat, nutrition, secretion, &c., — if he has not studied these important phenomena with the help of several of his senses, and especially his sight.

I would urge upon you, therefore, to make good use of those low creatures, endowed with so little sensibility, — the frogs, the fishes, and the turtles; to which list I might add the rabbits, animals whose sensibility is indeed so dull, that they will hardly stop eating a carrot (even when not particularly in need of food) while you are cutting their flesh; and who will return to their gastronomic occupation immediately after the division of their skin, or of one of their large nerves. I agree fully with the worthy motives of

the gentlemen composing the English and American societies for the protection of animals, so long as they only want to prevent absolutely useless and wanton cruelty to animals; but I must, and do radically, dissent from them in the attempts they have made in Europe and here to check if not to prevent altogether certain modes of scientific research, the great object of which is the good of mankind. I am selfish enough to prefer *mankind* to *frogkind*, *rabbitkind*, &c. I do not, however, recommend to you, Gentlemen, to shed much innocent blood. A small number of well-devised experiments made by yourselves or in your presence, by friends or by your teachers, would be sufficient. Let me say also, that, if you do not like to experiment on a warm-blooded animal, you can ascertain on frogs the truth of most facts of importance in experimental physiology or pathology, and in the science of the action of remedies and poisons. This is quite possible, because all vertebrated animals, cold or warm-blooded (as regards those organs existing in all of them), differ physiologically only by the extent of the vital properties of their tissues, and the functions of their organs, and not by the nature or quality of these properties and functions. You can, therefore, experiment only on frogs, or, if you prefer it, on turtles or fishes.

Sure as I am that you are full of good-will, I will suggest also that you should study upon yourselves the effects of the most valuable remedies. I believe

that you will never know fully the action of certain remedies, if you have not ascertained, on your own persons, what effects they produce on the brain, the eye, the ear, the nerves, the muscles, and the principal viscera. Pray, do not think that I ask of you too great a task. I need not say that you are not called upon to execute all I have mentioned in the short—alas! too short—period of time of the winter session. I hope you have understood that I am now giving you advice, not for the present only, but also for a near or a distant future.

It is important, if not essential, that you should study experimentally also the phenomena produced by the principal poisons. The curious tremulous movements caused by the chloride of barium or by the *Cocculus Indicus*, the peculiar tetanic spasms due to strychnine, and the various kinds of convulsions due to other poisons, you would never forget after having seen them.

I conclude these remarks by urging upon you the importance of studying practically every branch of the medical sciences, and not simply anatomy, medicine, surgery, midwifery, and chemistry.

The third rule to which I wish to call your attention, is, that not only should you practically study physiology and the mode of action of remedies and poisons, but also devote much more time than is usually given by students to those branches of the medical sciences. The world has considerably

changed since the time when William Harvey was considered as being crack-brained for having made the greatest of all physiological discoveries, and lost thereby a very large part of his income from the practice of medicine. The reverse is now the case; and it can be said, that in our age the most successful practitioners have almost all been physiologists. To prove the correctness of this statement, as regards Harvey's own country, I have only to name Sir Benjamin C. Brodie, Sir Charles Bell, Graves, Todd, Alison, Marshall Hall, among the dead; and Christison, Sir Henry Holland, Bowman, James Paget, among the living.

We live in a time when floods of new light are thrown daily on the mysteries of disease, and on the painful uncertainties of therapeutics, by the constant and great advances in our knowledge of structural anatomy, of physiology, and of the effects produced on healthy or morbid organisms by remedies and poisons, and also by food and spirituous liquors. A few illustrations will show how greatly important in the practice of medicine are the teachings of these branches of science.

Suppose a man comes to you with a paralysis of motion and sensibility in one side of the body, — let us say the *left* side. You examine his face, and you find that the angle of the lips on the *right* side is drawn towards the ear; and, if you are not learned in anatomy and physiology, perhaps you will stop there,

and consider the case as one of those so frequent instances of hemiplegia due to brain-disease, in which it is generally very difficult, and frequently impossible, to make out the seat of the disease in the cerebral organs. But, if you examine a little more, you find that the eyelids of the *right* eye either cannot be closed at all, or can only partially be closed; and also that the drawn and contracted muscles on the *right* half of the face do not obey at all the orders of the will, and are therefore paralyzed, while those of the *left* half are entirely obedient to the will; and now, examining the state of sensibility in the face, you find also that either pinching, pricking, applications of heat and cold, or any touch or tickling, are not felt or are very little felt. You have, therefore, to deal with a clear case of paralysis of motion and sensibility of the *right* side of the face and of the *left* side of the body. But the case is even more interesting than is shown by the above symptoms: the *right* eye is somewhat inflamed, and its cornea perhaps ulcerated; squinting and double vision exist also, and the sense of taste is lost or diminished on the *right* side of the anterior part of the tongue. If the patient sneezes or gapes or coughs, you may see his *left* limbs, which by his will he cannot move at all, fly about in the air, executing a powerful, rapid, and extensive involuntary movement. Besides, you may find that the movements of the tongue remain perfect, while the power of mastication, so rarely altered in hemiplegia, is lost or diminished on the

right side, owing to a paralysis of the masseter and other masticatory muscles.

A knowledge of anatomy and physiology easily and fully explains the symptoms of such a case, and also points out clearly that the seat of the disease is the lower part of the Pons Varolii, in its *right* side. That part contains the following nerves: the trigeminal, facial, and abducens, of the *right* side, affected there before they decussate with their fellows of the other side. It contains also the conductors of sensitive impressions and of the orders of the will to muscles, belonging to the *left* side of the body; conductors which, when they reach the Pons, have already made their decussation with their fellows of the other side.

Structural anatomy and physiology will make you understand, completely and easily, the otherwise most obscure case that I will now mention in a few words: A woman is stabbed nearly at the level of the interval between the seventh cervical and the first dorsal vertebræ. She is at once completely paralyzed of voluntary motion in the right lower limb; but, instead of having lost sensibility in that part, she has there a morbidly increased power of feeling heat, cold, a touch, a tickling, and pricking, pinching, &c. The left lower limb, on the contrary, although not affected in the least as regards either the force of, or the power of directing, voluntary movements, is absolutely dead to all impressions of heat, cold, touch, tickling, pinching, pricking, &c. But this is not all. On the

right side, her eye is evidently drawn back, the eyelids are partly closed, and the pupil constricted; several muscles of the face are in a state of contracture; the eye and ear are redder than the same parts on the other side; and the sensibility of the skin of the face is increased.[1]

Anatomy and physiology now explain easily all these symptoms. The whole right half of the spinal cord had been transversely divided a little above the level of origin of the nerve-fibres which go from that nervous centre to the cervical sympathetic nerve, and, along with it, to the blood-vessels of the face, to the iris, and to the orbital muscle of Heinrich Müller.[2]

Physiology teaches that an injury to certain parts of the medulla oblongata is soon followed by diabetes. Is it not clear that practitioners who know this fact will find that the diagnosis of an otherwise doubtful case of disease of that nervous centre is rendered easier by the existence of diabetes with the other symptoms that seem to show what is the seat of the disease?

A knowledge of physiology may also prevent your giving unfounded hopes to an afflicted family. I remember that once, just after I had left the bed-room where, a few minutes before, a much-beloved

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[1] I have published all the details of this case, in "Journal de la Physiologie de l'Homme," &c., vol. vi. p. 583.

[2] As regards anæsthesia, hyperesthesia, and other features of a section of a lateral half of the cord, I will refer to my paper in "Journal de la Physiologie," vol. vi. (1863–1865), pp. 124–145, 232–248, 581–646.

patient of mine had died of cholera, I was urged to return to his bedside, as it was supposed that he was not dead. I hastily did what was desired; and I witnessed a wonderful movement of the dead body's arms, both of which were raised up at a right angle with the trunk, as regularly as if led by the will, the hands advancing towards each other so that the fingers of one side came to cross those of the other side, as is done by Catholics in prayer; while at the same time the body, which was deathly cold during the last hours of life, had now, after death, become very hot. Had I not been taught by physiology what the signs of death are, I might have given hopes, soon to be painfully disappointed, to a distressed family; as I would perhaps have mistaken for voluntary movements the perfectly regular, and in appearance will-directed, movements performed by the upper limbs of that dead body.

Physiology teaches that transfusion of blood may be made in perfect safety with defibrinated blood, and that the blood of a dog or another lower animal may restore life in a man. Physiology has gone still further, and teaches also that in a dying patient, who has already lost consciousness, you can by transfusion restore life to a sufficient degree to obtain a return to consciousness, which may last long enough to allow the dying man to make a will or to give a parting word to his family and friends.[1]

[1] See "Journal de la Physiologie de l'Homme," &c., vol. i. (1858), p. 666.

It would be easy to give you many other equally decisive illustrations of the importance of physiology; but, as time presses, I will only say that there is no disease of any organ on which there has not been lately some light thrown by physiology; and I may add, that, owing also to the progress of this science, the treatment of all the affections of the nervous system, and of all the principal viscera, is now beginning at last to become rational, instead of being purely empirical as it was. The time is near at hand when the teaching of the diseases of any organ, and of the therapeutics of these diseases, will become absolutely inseparable from the physiological history of the same organ.

What I have just said of physiology is true also as regards the study of the action of remedies and poisons. A few facts may prove that you should devote more time and attention to this study than students usually bestow on this branch of the medical sciences. The facts I will mention are very little known, notwithstanding their great practical importance.

When the dose of a poison is not such as to cause death at once, or in a very short time; and when, also, the toxic agent is not of the class that destroys life by an evident alteration of an important organ, — it is easy to fight successfully against the action of the poison by means which have been discovered through a physiological study of that toxic action.

In the first place, I have found that death is often due to the lowering of the temperature of the body, so that you may save life, in many such cases of poisoning, by employing the proper means of maintaining or raising the degree of animal heat. In the second place, there is a very good chance, as first shown long ago by Sir Benjamin Brodie, that the poison will go out of the system by some secretion, if by artificial respiration you keep the patient alive long enough for the expulsion of the toxic agent through a gland. There are several other rational indications which show that you should try to increase all the secretions in cases of poisoning; and you would learn these important things by studying, more than is usually done by students, the physiological history of poisons.

Again: the importance of a knowledge of the properties of remedies, more extended than that generally possessed by practitioners, is well illustrated from the effects of treatment of paralysis of the lower limbs by strychnine, or by the ergot of rye and belladonna. These three most powerful remedies, when properly applied, can do much good, while they will increase the paralysis if wrongly used. Strychnine will produce hyperæmia in the spinal cord and its membranes, while the two other remedies will diminish the amount of blood in those parts. This fact being known, and the symptoms of congestion and those of anæmia in the cord and its meninges being known also, it is

easy to detect in what cases we should employ the first of these remedies, and in what other cases we should make use of the two last remedies.

The remarks I have made heretofore apply evidently to all the students who now hear me. I will now make some remarks which may appear to be addressed only to a few, but which, I hope, will, in reality, prove useful to almost all, if not to all, my young hearers.

Some among you, Gentlemen, are more ambitious than others, and have the noble wish to rise above the ordinary level of the profession, by giving an impulse to the progress of some branch of the medical sciences. A great many more would also have the same wish, and would accordingly work from the very beginning of their student's career, if they only knew that the task is not so great as they imagine it to be, and also that they may reap a much richer harvest than they might now hope for. It may be useful, on that account, to remind you that the mental faculties which in eminent men shine so brilliantly, exist also in most other men; and that exercise, in appropriate ways, will make these faculties grow. I certainly do not believe, with Thomas Young, that all men can do what another man has done, as I know that there are faculties altogether missing in some individuals: still it is quite certain that most people have some degree of the faculties essential to scientific or practical researches. In sciences of obser-

vation like ours, it is not necessary to be endowed with a peculiar mental disposition to become a discoverer. Develop, by frequent use, the faculties that exist in you; and, before very long, you will find, in applying your mental power to the progress of science or to that of practice, that you are surrounded by discoveries to be made, that they press upon you on every side, and that what is essential to make them come out, and thereby to benefit science or practice, is your earnest effort to see what is under your eyes. In every thing the power of training is immense. Train your memory, and you may soon be surprised at the wonderful increase of its power. I admit that in some persons there will be a much greater increase than in others; but it will take place in all, as you may learn from stage-actors. Train your mind to a careful observation of facts; and before long you will be pleased to find, that you detect the details of facts very much quicker, and with much more accuracy, than ever before. Train your mind to draw conclusions, and also to study critically those which have been drawn by others, and you will soon be able to point out many scientific errors, and to discover new truths. Have we not abundant proofs of the correctness of these assertions, in every career, in every art, in every science? Take as an example the remarkable feats accomplished by that wonderful prestidigitator, Robert Houdin, and particularly the following: He trained himself to become able to juggle with four balls at

once; but, not being satisfied with this performance, he accustomed himself to read without any hesitation while the balls were in air. He who has read the interesting remarks of Henry Ward Beecher, on the amount of progress that a man must make to become a quick setter of types, will admit easily how great is the power of training in the art of printing. You will find, if you look for them, a great many examples proving the same thing as regards every one of our physical, sensorial, or mental faculties.

Let me repeat, that it is not so difficult as you may imagine to make discoveries in the medical sciences. Without any wish of diminishing the merit of one of the most ingenious discoverers in physiology in our time, I will point out to you how easily he made the discovery of the important fact, that, in certain circumstances, the liver is gorged with sugar. One day he decided that he would test for sugar every organ in the body; and in a very short time, and with very little labor, he ascertained that, except the liver, there is either no sugar, or only an extremely small quantity of it, in all the organs of the body. This being discovered, he examined the portal blood near the liver, and found that it contains no sugar; while, on the contrary, in testing the blood coming out of the liver, he found it very rich in sugar. Hence the theory, — now proved false, but which nevertheless has been most useful to science in opening a new field, — that it is a function of the liver to make sugar.

The first duty of him who wishes to help science or practice is to avoid committing certain kinds of error which have thrown so much discredit on our profession. With the double purpose of warning you against the most serious and frequent of these errors, and of showing you how easily it would have been, even to men of very small intellectual power, to help science in detecting them, I will point out a few of the most notorious.

The first kind of error that I will mention consists in *admitting that a theory is demonstrated, simply because some able authors declare it to be so.* There is one very remarkable instance of that kind of error, in the general admission, by the ablest physiologists and medical practitioners of our time, that Galen has proved that the conductors of sensitive impressions, as well as the conductors for voluntary motion, do not decussate in the spinal; and that consequently an injury to one of the lateral halves of that organ produces a paralysis of both sensibility and motion in the corresponding side of the body. Had any one taken the very slight trouble — if it is at all a trouble — of reading what Galen says, in two places of his works,[1] on this subject, he would have found at once that that great vivisectionist and physician does not say a single word as regards the state of sensibility in the hogs on which he performed his celebrated experi-

[1] De Locis affectis, lib. iii. cap. xiv.; and De Anatom. administrationibus, lib. viii. sect. vi.

ments on the spinal cord. The vitality of an error, when once admitted as truth, is so great, that, up to a very recent time, notwithstanding most decisive facts observed in our species, and not less decisive experiments on animals,[1] it was held, and is still held by a few physicians, that the sensitive impressions coming from one side of the body are transmitted to the sensorium by the same side of the spinal cord; and this is partly grounded on the authority of Galen, who in reality, as already stated, does not say a word about it!

The second kind of error that I will mention consists in *continuing to accept a theory, notwithstanding our recognition that there are positive facts in direct opposition to it.* In this respect, there is a theory which is in evident contradiction with almost every positive fact that we know as regards brain diseases, and which is still generally admitted, although it would be easy to the least advanced student in this class to demonstrate its utter falseness. Mark, if you please, that this theory has always been a stumbling-block in the path of progress; and that we owe to it the extreme slowness in the advance of our knowledge concerning the mode of production of symptoms in brain diseases.

This theory is, that in organic affections localized in

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[1] For the clinical and the experimental demonstration that the conductors of sensitive impressions decussate in the spinal cord, see my work, "Course of Lectures on the Physiology and the Pathology of the Central Nervous System," Lectures III. and VII.; and "Journal de la Physiologie de l'Homme," &c., vol. vi. pp. 124, 232, 581.

a part of the cerebral lobes, when symptoms are not due to a pressure upon distant parts, they are the consequences or the direct effects of a loss, a diminution, or an alteration of the functions of the diseased part. It would not take long for any one here to ascertain, that in cases of softening due to a congestion or an inflammation, and limited to a small part of the cerebral lobes, — cases in which there is not the least pressure on distant parts from the diseased one, — the symptoms cannot be due to a loss or an alteration of the function of the diseased part. In the first place, it is easy to ascertain quickly that there are on record cases of that kind (even sometimes with an abscess), without the production of any symptom whatever. It is easy, also, to find quickly other cases of a local organic affection of the cerebral lobes, without any pressure on neighboring parts, giving rise to any of the symptoms of brain disease; showing that the greatest variety, as regards the number, the kind, the peculiar grouping, the intensity of symptoms, may be noticed in cases of a disease of the same small part of the cerebral lobes. I hardly need to say, that if the symptoms were, as the theory teaches, effects of the loss or of an alteration of the functions of the diseased part, you would have to conclude, from some facts, that the various parts of the brain proper have no function whatever (as shown by those cases in which, although the part is quite destroyed by disease, there is no symptom at all); and,

on the other hand, from other facts, you would have to conclude also that each one of the same parts of the cerebral lobes is proved, by an immense number of cases, to be endowed with all the properties, all the functions, of the whole brain (as shown by those cases in which the greatest variety of symptoms is exhibited). I repeat, that you would have to admit that each small part of the brain proper possesses every one of the large number of properties and functions of the whole brain; while, on the other hand, you would be led to maintain that there is no part of the cerebral lobes at all endowed with any property or function. Is it not humiliating, Gentlemen, that a theory leading to such palpable absurdities is allowed to stand unchallenged by the ablest men of our profession?

A third kind of error, which you can easily dispose of, consists in *considering the whole brain as one organ, while it is clearly composed of very widely different organs.* What would you say of a physician who would call by only one name the inflammations of all the various organs of the abdominal cavity, and who would describe the symptoms in this way? "In inflammation of the abdomen, there is sometimes diarrhœa, in other cases, constipation; there is sometimes an increase, in other cases a cessation, of the secretion of bile, with or without jaundice and nervous symptoms: there is sometimes an increase, in other cases a diminution, of the urinary

secretion; and the urine may be found either normal or albuminous or chylous or bloody, or charged with pus, mucus, casts, &c. Besides, we observe sometimes epileptiform convulsions, or hemiplegia, or coma," &c., &c. So that all the symptoms of inflammation of the liver, the kidneys, the bowels, &c., would be given as being the symptoms characterizing one disease, one inflammation!

Now, Gentlemen, you may be surprised to hear it: but something equivalent to such a monstrous mixing up of radically different affections is being done for the brain; and, in the history of inflammation, or tumor, or hemorrhage, &c., of that nervous centre, symptoms are given which vary as much according to the seat of the disease as hepatitis, nephritis, enteritis, &c., differ from each other. Imitating Abercrombie, who collected cases of tumor in almost all parts of the brain, and came to the sad conclusion that the symptoms were too widely different to lead to a diagnosis, eminent writers have even recently tried to make out the symptoms of cancer, of abscess, of cysticerci, of the brain, by bringing together cases not at all comparable one to the others, owing to the fact that the seat of the disease was in radically different parts of that nervous centre. The result, of course, as should have been anticipated, has been that a diagnosis is almost always impossible. What is to be done is to study the symptoms of disease located in one part of that great collection of organs, the brain, and then to try to

discriminate between symptoms of congestion, of inflammation, of softening, of tumor, &c., of that part.

The fourth kind of error that I will mention consists in *admitting as true, and without any criticism, what is sanctioned by learned societies.* A good illustration of this kind of error is to be found in the universal admission of Longet's views concerning the transmission of the orders of the will to muscles, and of sensitive impressions to the sensorium, along the spinal cord. That theory was full of contradictions; but the confidence in the authority of the members of the French Academy of Sciences, who had given to it their sanction, led every one to admit the views of Longet. For several years there was no criticism; and the faith was so great, that even positive facts showing the utter worthlessness of the theory were disregarded. At last the many contradictions of the theory were pointed out,[1] and it is now completely abandoned. With a little common-sense, and a decided unwillingness to admit, as necessarily true and correct, statements and opinions published or approved by men in high places, any one might have prevented science from being misled, and its progress delayed, by Longet's flagrantly contradictory, and therefore untenable, views.

The fifth kind of error I intend mentioning is not so easily detected; and it is therefore more frequently

[1] See Course of Lectures on the Physiology and Pathology of the Central Nervous System, p. 15.

committed, either by experimenters on living animals or by medical men in their clinical researches. It consists in *neglecting certain circumstances of cases or experiments, and of drawing, consequently, erroneous conclusions.* A most egregious mistake of that kind will have a prominent place in the history of Toxicology and Materia Medica. It was committed by the celebrated toxicologist, Orfila. He made researches, upon dogs, on the influence of a very large number of salts and other substances. He injected into the stomach the substance to be studied; and, to avoid its being rejected by vomiting, he used to tie a ligature round the œsophagus. Not taking into account the effects that might be caused by the irritation applied to the nerves of that very sensitive tube, he was led to admit that most of the substances he experimented with were toxic, even in doses that are not very large. It is somewhat surprising that he was not led to recognize some cause of error in his experiments, from the fact that very widely different substances seemed to produce nearly the same symptoms. Notwithstanding this fact, his conclusions went on unchallenged for a good many years; and, up to this day, the books on Materia Medica and on Toxicology are full of gross blunders that are due to that fatal mistake of Orfila. About ten years ago, however, it was found that the phenomena which had been considered as due to the action of a great variety of salts and other substances were, in reality,

effects of the tying of the œsophagus; and it was then shown that there are very excitable incito-reflex nerves in the œsophagus, and that an irritation of these nerves by a ligature is quite enough to produce a variety of natural and morbid reflex phenomena, which Orfila mistook for toxic effects of many of the substances on which he experimented.[1]

The sixth kind of error I will bring to your notice occurs much more frequently than the preceding. It consists in *denying facts because we cannot explain them.* To avoid this error, what you have to do is to ascertain positively if the so-called facts are really *facts*; and, if you do recognize them to be incontestably so, you must of course admit their existence, notwithstanding their opposition to current theories or your inability to explain them. I remember the laughing outcry which ran through the medical profession in France, when it was announced that for centuries sciatica had been cured, in the island of Corsica, by the cauterization of the helix of the ear. But a man of courage and independence of thought, the late Professor Malgaigne, ascertained it to be a positive fact that the application of the white-hot iron to the ear sometimes cures sciatica.[2] In this instance, as in

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[1] See Journal de la Physiologie de l'Homme, &c., vol. i. (1858), pp. 777 and 809.

[2] An Italian physician, G. Finco (see "Year-book of Medicine," &c., of the New Sydenham Society, for 1863, p. 99), states that he has employed that mode of treatment in forty-eight cases of sciatica; in thirty with complete, in ten with incomplete, and in eight without any, success. The cure takes place through a reflex influence.

others in which real facts have been denied to be such, not only is it important, or at least interesting, to know the truth, but you are to expect a great deal more for the progress of science from facts in opposition to received theories than from facts giving them a confirmation. A fact in antagonism with admitted views not only leads to the abandonment of erroneous views previously considered right, but opens a new field which may be of considerable importance to science and to practice.

It would be easy to point out a great many other kinds of error; but I will not do so, because our time is limited, and also because I hope what has been said is enough for me to draw the conclusion I wished to lead you to. I hope you will admit with me, that, in the instances of errors which I have mentioned, a little common-sense, together with the will of not being deceived, would have quickly led to the upsetting of the views and conclusions based on the errors I have spoken of; and that science and practice would consequently have progressed faster than they have. You will allow me to add, that, if our professional brethren considered it a duty for them to employ their common-sense for the good of the medical sciences, the profession would soon occupy a much better position in society than that which it now occupies.

These last remarks lead me naturally to a kindred subject. I often hear physicians and students express a great wish to do something for the progress of their

art, but declaring at the same time that they do not know what easy questions are to be solved. It seems wonderful to me that they have the least difficulty in finding a subject to work at without much trouble. Is there a question which, as soon as it is solved, is not at once followed by others of greater importance, and opened by the very solution of the first? The field of what is unknown, far from being diminished by the progress of our knowledge, is constantly enlarging. Behind the small island first discovered by Columbus there was a great continent; and so also in the progress of science, behind each small bit of discovery stands an immense extent of discoveries to be made.

But it is said that scientific researches are long and difficult, and they bear fruit only after hard and prolonged labor; and that therefore a busy practitioner, and still more a busy student, cannot do any thing for science. There is certainly some truth in this; but let me ask you a few questions: —

Are not the busy practitioner and the student in a hospital constantly in presence of facts which have not yet been discovered, and which can easily be discovered?

Are they not constantly the witnesses of phenomena disproving great theories which, by being false, are obstacles to the progress of science?

Is it very difficult for them to collect facts on that great question (still discussed, although with others I think it is quite solved), of bleeding in inflammatory

diseases, or in cases of pulmonary or cerebral hemorrhage, &c.?

Is it very difficult for them to verify what is stated of a new remedy, or of a new property of an old remedy; and to examine into the circumstances that seem to favor or to prevent the good influence of these remedies?

Is it difficult to ascertain what changes are produced, under the influence of disease or remedies, in the tactile sensibility of patients, as measured by the compasses; and in their voluntary muscular strength, as measured by a dynamometer?

Is it not a part of the duty of the busy practitioner, and even of the student, to ascertain what are the symptoms of cases under the care of the first, and the observation of the second; and, if they perform that duty, are there not a great many questions that they can solve, — for instance, by showing the connection of a certain symptom with a certain organic alteration?

Is it such a hard task to ascertain what truth there is in the stated antagonism between phthisis and fever and ague, or what is the extent of the influence of dampness of the soil on phthisis; or to record what are the prevalent diseases of the part of the country where one practises medicine, giving materials to establish a geographical nosology?

Cannot they compare the mortality records of this country with those of Europe, showing how the climate acts here on the human race?

Cannot they prove how much mankind is indebted to the medical profession, in showing how immensely different is the mortality of people living in civilized countries, comparing what it is now with what it was a century ago, from the ague, dysentery, scurvy, childbirth, small-pox, &c.?

Cannot they at least, if they do not make discoveries, prevent great advances of science from being set aside; such as was the case with the great discovery of the reflex phenomena, fully made by Prochaska, Unzer, and Whytt, in the last century, and neglected or forgotten for more than fifty years?

One more question, and I have done on this point. Cannot the busy student, born and raised on this continent, find some little time to give full information about his place of birth, his parentage, his height, his weight, the average frequency of his pulse, and thereby help to the establishment of laws disproving those statements of European critics, that the human race is degenerating in North America, — statements in which they persist, notwithstanding the wonderful display of physical strength, of power of endurance, of courage and determination, exhibited all through the late sanguinary war?

In the field of experiments on living animals, there is so much to be discovered by so little work, that I cannot understand why there are not more vivisectionists. Indeed, when we think of the complete revolution which is now taking place in every branch

of the science of medicine, from two very easy experiments, — one consisting in a division of a nerve; the other, in the irritation of the same nerve by galvanism, — it is wonderful that young men, ambitious to do good, are not attracted in large numbers to a field of research where so much can sometimes be found by so little effort.

But, I am asked, how can we learn to make scientific or practical investigations in physiology or medicine in a country like this, where the teaching of these sciences is yet only rudimentary? I do not deny that there is a great need here of an institute, where the means of prosecuting scientific researches should be taught by competent men. I have no doubt that he who would establish such an institute would be a benefactor of the human race, not only in this Republic, but all over the world; while he would also do much, at the same time, to place this country on a level with Europe, for things about which the inferiority of America is notorious.

Admitting all this, however, I cannot but say, that, if you are seriously willing to work, you can dispense with any special teaching; and, what is still more important, I am sure that you must succeed. If you have the will, you will soon find that scientific or practical facts abound; and that you have only to open your eyes, your ears, and your hands, then to pay attention to the teachings you have received through your senses, and at last to draw conclusions.

Have the will, I repeat, and you will soon help science; to which I might add, that you would also do much for your own personal success, if I did not dislike appealing to selfish motives. Indeed, the difficulty is not what to do or how to do. It is that the mine is so rich, so attractive on all sides, on every point, that it is difficult not to spend time, one day in one field, the next in another; and, from lack of concentration of efforts, to fail to produce any very great work.

Now, Gentlemen, in concluding this address, I feel that I must apologize for having carried you through such a long array of arguments, and I give you my best thanks for having listened so attentively to my remarks.